of the Garden and School. The present volume, therefore, really begins the series of annual reports, and together with the reports we have a revision of the North American species of *Epilobium*. In the earlier part of the book details are given of the appointment of six garden pupils to scholarships in accordance with a resolution adopted by the trustees at a meeting held in November 1889. Each scholarship conferred may be held by the recipient for a period not exceeding six years, subject to certain conditions. The holders of scholarships are repaid for their services to the Garden, and at the expiration of the six years are entitled to examination by the Garden Committee. On passing such examination to the satisfaction of the Committee and Director, they receive a certificate of proficiency in the theory and practice of gardening. The only scientific paper in the volume is, as we have just mentioned, a revision of the genus *Epilobium*, the American species occurring north of Mexico being those studied. This genus differs from all the other capsule-bearing *Onagraceæ*, except the Californian *Zauschneria*, in having its seeds provided with an ample coma at the apex. While it reaches great development in New Zealand, *Epilobium* is essentially a genus of temperate and cold climates, and the most widely distributed species are those of Arctic and Alpine regions. In Alaska a few such species occur, which are otherwise confined to the adjacent part of Asia. More widely distributed Arctic-Alpine immigrants from the Old World to the New are *E. spicatum*, *E. latifolium*, *E. palustre*, *E. alpinum*, &c. *E. hirsutum*, *E. parviflorum*, and *E. adnatum*, also occur as accidental waifs. The genus passes into South America along the backbone of the continent; few members of this family extend very far across the Mexican boundary in either direction. The most interesting biological features of the genus are those connected with the means of vegetative propagation, pollination, and dissemination. The contrivances by which species survive the winter, and are vegetatively propagated, in this respect attain an extreme degree of differentiation, one in particular having acquired aërial bulblets. The large-flowered species appear to be regularly proterandrous, the duration of the dichogamy being brief in most of them, and the smaller-flowered seem to be always synacmic and self-fertile, although with the probability of frequent intercrossing by aid of insects attracted by the nectar which is secreted within the calyx tube. The genus is of no striking economic value. The North American Epilobia have been mostly described by De Candolle, Torrey and Gray, Haussknecht and Barbey; the more notable works of more limited range being Hooker's "Flora Boreali-Americana," and Brewer, Watson, and Gray's "Botany of California." Prof. Trelease in his revision enumerates 38 species, which number includes the following novelties: *E. holosericeum*, *E. delicatulum*, and *E. clavatum*. The well-known sections *Chamænerion* and *Lysimachion* are still adhered to, the latter, of course, being by far the larger. In the analytical key the main divisions depend on whether the stigma is deeply 4-lobed or 4-cleft, or entire or only notched. Subdivisions are founded on whether the seeds are smooth, or papillately roughened. The name *E. spicatum*, Lam., is used instead of *angustifolium*, the typical *angustifolium* of Linnæus being, according to Prof. Haussknecht, what is commonly known as *E. Dodonæi*, Vill. We are glad to see that Prof. Trelease differs from Prof. Haussknecht in not adopting a new name for what is left of the original *E. alpinum* The *E. alpinum* of Linnæus included with this *E. Hornemanni* and *E. anagallidifolium*, but we think that the name may well stand for one of the segregates. The genus *Epilobium* has always proved a difficult subject; and Prof. Trelease is to be congratulated on his careful treatment, and successful arrangement, of the North American members. The 48 plates will be found of great help to students of these plants; they are not quite of uniform merit, but, taken as a whole, they give the essential details, stress being laid on the varied form of the stigma and seed. Additional illustrations are some well-reproduced photographs taken in the Garden, and a plan of the grounds (scale $\frac{1}{720}$) in five sections.

E. G. B.

OUR BOOK SHELF.

The Story of the Heavens. By Sir Robert Stawell Ball. Eighteenth Thousand. (London: Cassell and Company, 1891.)

IN the preface to this edition, Sir Robert Ball remarks that he has taken the opportunity to "revise the work in accordance with the progress of astronomy during the last four years," and, generally speaking, new facts and theories are briefly referred to. A few points, however, are hardly brought up to date. For example, the spectrum of the Andromeda nebula is said to be "a faint continuous band of light" (p. 462), although it is now definitely known that this continuity does not exist. We also find no reference to the many stars now known to have bright lines in their spectra. The author thus misses a chance of exercising his well-known descriptive ability in an account of the connection between such stars and nebulæ; the similarity of the two being so considerable that Pickering has followed Lockyer in arranging them in a single group. Dr. Huggins's old view as to the coincidence of the nebula line with nitrogen is mentioned merely to be dismissed as erroneous. Why, therefore, is no notice taken of the suggested magnesium origin of the line—for, on any published evidence, the edge of the magnesium fluting is nearer the proper position than the nitrogen double? We would also point out that, according to recent observations, the apex of the sun's way is much nearer Lyra than Hercules. Telescopic changes in comets are fully described, but the accompanying changes in their spectra are not touched upon. Motions of stars in the line of sight are considered; but not those of nebulæ, although Mr. Keeler's observations have been published for some time. In fact, it may be said that there is a tendency to eschew spectroscopic questions, and hence much of the most beautiful part of the story of the heavens is left untold.

Notes on Elementary Physiography. By Horace C. Martin. (London and Manchester: John Heywood, 1891.)

THE author has collected a lot of scraps of information from standard writers on physiographical matters, and has strung his gleanings together to form this book. And if he were an adept at compilation, and knew how to best arrange and connect facts, this plan of printing extracts *verbatim* might be commended. But when Mr. Martin selects notes which by themselves are incorrect, and inter-

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polates in others crude statements which render them ridiculous, he does an injustice to the authors to whom he acknowledges his indebtedness, and he shirks responsibility by saying that "these notes do not lay claim to originality." Could anything be more misleading than the following description of sun-spots on p. 148? "They seem to rise suddenly to a great height, cool, and then sink back into the photosphere. They are due to uprushes of incandescent hydrogen, and are identical with the red flames seen during an eclipse." And the figure that accompanies this text cannot be a sun-spot at all, but must be something else inserted by mistake. Another blunder occurs on p. 59, where a section of an intermittent spring is shown upside down. The figures are mostly very coarse and poor, especially the moraines on p. 62, the section through a cinder cone on p. 89, and one of a volcano on p. 90; whilst the two figures of ocean bottoms on pp. 102 and 103 give a very wrong idea of their nature. There is, of course, a deal of information in the book, but no attempt is made to give it interest. In fact, although the author is a teacher of physiography, it is very evident from his work that he has not paid attention to the practical side of his science, or verified any of the phenomena he essays to describe. As a book of reference the work before us is untrustworthy; and as a work for students of elementary physiography it is useless and much to be condemned.

Thomas Sopwith, M.A., C.E., F.R.S.; with Excerpts from his Diary of Fifty-seven Years. By B. Ward Richardson, F.R.S. (London: Longmans, Green, and Co., 1891.)

MR. SOPWITH died in 1879 at the age of seventy-six. He was not eminent as an original scientific investigator, but he was a man of great vigour and freshness of mind, and had won the affection of a wide circle of friends by his genial and happy temper. For many years he resided at Newcastle as an engineer and railway surveyor. Afterwards he removed to Allenheads, where he served as the chief agent of Mr. T. W. Beaumont's lead-mines in Northumberland and Durham. Dr. Richardson's book will recall Mr. Sopwith vividly to the minds of his friends, and it contains many things which will be of interest even to readers who were not personally acquainted with him. During the long period of fifty-seven years he kept a diary regularly; and of this, of course, Dr. Richardson has made liberal use. The extracts show that Mr. Sopwith studied closely the currents of scientific opinion, and formed his own judgment about them in a shrewd and independent spirit.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Electric Transmission of Power.

YOUR article of the 1st inst. on the International Electrical Exhibition (p. 522), says: "In those days (before 1879) two wrong notions misled people—the one, that the maximum efficiency of a perfect electromotor could be only 50 per cent.; the other, quoting the remarks of Sir W. Siemens, 'in order to get the best effect out of a dynamo-electric machine, there should be an external resistance not exceeding the resistance of the wire in the machine.'"

These two notions are really one: the first follows by immediate inference from the second.

Your article says a little further on: "At the British Association in 1879, Prof. Ayrton exposed the fallacy of assuming that 50 per cent. was the maximum efficiency theoretically obtainable from an electromotor. . . . This was perhaps the first time that it had ever been suggested that the efficiency in electric transmission of power could be more than 50 per cent."

This is a mistake as to historical fact. Many years ago, I am not sure of the date, but it was long before the dynamo was invented, I had some conversation with the late Prof. Joule about mechanical equivalents and motive power, in which he told me that an electromotor (worked, of course, by a voltaic battery) had shown a very high percentage of efficiency—I think he said 79 per cent., and I am sure it was far above 50. I said, "How is that compatible with Ohm's demonstration that the efficiency of an electric circuit is at a maximum when the resistance of the battery is equal to that of the rest of the circuit?" to which he replied, "The maximum effect, in Ohm's theorem, does not mean the maximum work done by the oxidation of a given quantity of zinc, but the maximum effect obtainable from a given surface of zinc plates." "I see," said I, "just as in the case of the steam-engine, the problem of getting the maximum of useful effect from a given weight of coals is a different one from that of getting the maximum of power from a given area of piston."

This appears to be an instance of a truth being grasped by one of the great masters of science long before it passed into general teaching. And it is also an instance of a truth being so mistaken as to mislead: Ohm's law was evidently understood to bear a significance that it did not really bear.

Belfast, October 13. JOSEPH JOHN MURPHY.

[That Joule had clear and correct views regarding the efficiency of an electromotor driven by a voltaic battery was pointed out some years ago, being mentioned, for example, by Prof. S. P. Thompson in his book on "Dynamo-Electric Machinery." But in the paragraph quoted by Mr. Murphy from NATURE of October 1, the expression "electric transmission of power" had reference to the combination of apparatus exhibited at the lecture in question—had, in fact, the meaning usually attached to this expression, viz. the employment of a dynamo to convert mechanical energy into electric energy at one end of a pair of wires of some length, and the employment of a second dynamo at the other end of the wires to convert the electric energy back again into mechanical energy.

Now, not only would it have been somewhat difficult to foretell what would be the combined efficiency attainable by the employment of two dynamos as generator and motor, at a period "long before the dynamo was invented," but even down to 1879 no one had succeeded in practically transmitting power by means of this combination with an efficiency of as much as 50 per cent. over a distance of even one mile.

The only direct-current dynamo in common use at that date was the series-dynamo, and that machine, as is well known, differs radically in its behaviour from a voltaic battery. For while it is when a voltaic battery is developing a very small current that it gives power most economically to the outside circuit, the series-dynamo, when only a very small current is passing through it, develops practically no electromotive force, no power, and therefore has a very low efficiency. Hence, although electricians were undoubtedly mistaken in fancying that there was a theoretical limit of 50 per cent. in the efficiency when two dynamos were employed in the transmission of power, neither the error, nor its correction, were of that obvious character in 1879 that one might imagine from reading Mr. Murphy's letter.—W. E. A.]

Rain-making.

IN 1883 I published in NATURE (vol. xxviii., p. 83) an account of some experiments which I made to explain the curious phenomenon commonly seen at the Bocca of the Solfatara of Pozzuoli: paper or brushwood is kindled near the fumarole, and the action of the flame, even when its duration has been very brief, is observed for some time after in the relatively great increase of cloudy vapour that appears to roll out of the Bocca and to rise from the surrounding minor fumaroles. According to Prof. Arcangelo Scacchi, this increasd condensation of vapour is due to the carbon dioxide produced in the combustion; this gas causing condensation from the highly saturated medium in the same way as fumes become visible when concentrated hydrochloric acid is exposed to ordinary air. My experiments of 1883 tend to show that not only carbon dioxide, but (in accordance with the views of Dr. Aitken on the formation of cloud or mist) the increase of solid corpuscles made to float in

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